

CHARLES EDOUARD BROWN-SÉQUARD (1817-1894)*

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The life of Brown-Séquard so approaches the fantastic in the ups and downs of his fortunes, in his incessant wanderings back and forth between two continents, in his prodigious, almost frenetic activity that it does not lend itself to orderly description; one is tempted to treat his life by charts and maps and by tables of statistics. In 32 years he published 301 papers; in his lifetime more than 500. He made his residence in America four different times, in the country of his birth twice, in England once, and he re-established his life in France six separate times. He occupied four chairs of physiology and refused as many more, he founded, edited, and partly filled three journals, and was married three times. He is said to have crossed the ocean 60 times and to have answered as many letters in a day. Perhaps all of this is made somewhat reasonable by his habit of working on boats or wherever he found himself, and of beginning his day at the unlikely hour of three in the morning.

Charles Edouard Brown-Séquard was born April 18, 1817, on the improbable island of Mauritius half a thousand miles east of Madagascar. Born the posthumous son of an Irish-American sea captain, a native of Philadelphia, he later added to the name of Brown the family name of his mother, Henriette Perrine Charlotte Séquard. By birth a Mauritian he was genetically Irish and French, or Galway and Provençal. The mysterious disappearance of his father at sea a few months before his birth precipitated the family into a period of hardship and privation which was to last for the remainder of his mother's life and half of his. At fifteen Charles Brown entered as a clerk a large importing establishment. Of his early education little is known beyond that it must have been scanty to a degree. At the age of twenty, when he persuaded his mother to migrate to Paris, began a remarkable parallel between his life and that of Claude Bernard. But as with Bernard it was

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not the study of medicine or the Universities that called him to Paris, but the ambition to become a writer of Belles-Lettres, at which he had for some years been trying his hand. Like Bernard he presented himself to a prominent literary man, Charles Nodier, and received a similar discouragement. Though without any resources he decided to make himself a physician and at once put himself under Martin-Magron to repair the defects in his education and prepare the two baccalaureates then obligatory. His studies were made possible by the strenuous efforts of his mother who, in the "genteel" language of the *Dictionary of National Biography*, "shared her house with the sons of some other Mauritian then studying in Paris."

In his second year in medicine he first manifested his interest in the subject which he was to pursue so vigorously, and in the laboratory of his teacher he was accustomed to repeat all of the experiments of which he had knowledge. Nicely launched on his studies, illness resulting from a *pique anatomique* lost him several months and soon afterwards the premature death of his mother, to whom he was deeply attached, demoralized him completely and sent him on a blind, nostalgic flight to his native island. This was the first of his many wanderings.

In 1846, eight years after coming to Paris, he stood successfully for the degree of Doctor of Medicine and defended a thesis titled, *Recherches et expériences sur la physiologie de la moelle épinière*. In his thesis of thirty pages were embodied two observations, first, that the reflex activity of the spinal cord falls nearly to zero after separation of the brain from the spinal cord, which then gradually recovers its reflex excitability; and, second, that the sensory impulses of the spinal cord are conducted chiefly by the gray matter of the spinal cord and not by the posterior columns. It is perhaps here rather than in Marshall Hall's celebrated paper four years later that we have the first unmistakable description of spinal shock, the current impression to the contrary perhaps arises from the fact that Marshall Hall named the phenomenon 'spinal shock,' a doubtful service to physiology. The second part of his thesis, is the more bold because in it he attacked an established doctrine dating from Charles Bell. In it he set a theme which runs through his later work and which has caused a syndrome to be named for him.

From 1846 to 1849 he was 'externe des hôpitaux' under

Trousseau and Rayer, and in 1849 became auxiliary physician under Baron Larrey at the military hospital, Gros-Caillou. In 1848 he joined a group of young scientists which included Claude Bernard who, under the leadership of Rayer, founded the *Société de Biologie* of Paris. He served as one of the four original secretaries in the founding of this great organization which represented the protest of youth against the formality and pomposity of the *Académie des Sciences*.

His position in these years was not uncommon but none the less unenviable. His poverty was extreme and his prospects little better. Through poverty he was forced to occupy miserable quarters, unheated in winter, and shared with the rabbits and guinea-pigs on which he experimented. Interested only in experimental science he was forced, in order to live, to devote himself to clinical medicine. His chances for a teaching post were reduced by the fact that he was a foreigner and a Republican, ardent to the degree of bearing arms in the current civil strife. His was the position which to this day exists in certain branches of science in France, particularly in psychology, and which still causes professors to speak in the vein of the following remarks made by Claude Bernard with Brown-Séquard in mind.

How sad was the future of those entering experimental physiology when through special circumstances they were not able to find a place in one of the public institutions. I have known some who despite their taste for physiological studies have shrunk from such obstacles, and others who, despite their passion for physiology, have been defeated in the struggle and have been obliged to change the direction of their studies or to leave France.

The latter was the course taken by Brown-Séquard. Arriving in New York with only the English that he had been able to acquire by the expedient of taking passage on a slow sailing-vessel, he was unsuccessful in obtaining an association with a medical school. As his friend, Dupuy has said, "it was no more possible in America than in Europe to live on the emoluments of pure science." He was forced to give lessons in French and to practice midwifery for the reasonable sum of six dollars per case. His position was improved by collaborating in the preparation of a successful treatise on obstetrics and he was able to carry on his experiments. I have been unable to discover the exact name of this work. During this period he

made the first observations on the stimulation of the cervical sympathetic, which in the minds of some entitles him to be considered with Claude Bernard a co-discoverer of the vasomotor nerves. Five months after Bernard discovered that section of the cervical sympathetic caused a flushing and warming of the skin of the head, Brown-Séquard performed for the first time the complementary experiment in which he showed that stimulation of this nerve caused constriction of the skin vessels. It was not consistent with their respective characters but it was Brown-Séquard who gave the interpretation which we now hold to be correct. It is interesting that both men, peering at the ear of their rabbit, failed to notice or at least to comment upon the changes in the pupil and other structure of the eye.

In 1853 he married an American woman and returned to France. A year later he returned to Mauritius with the idea of practicing and found there an epidemic of cholera, in which he served so well that a gold medal was struck in his honor by the grateful citizens. In 1855 he returned to America to fill the post of Professor of the Institutes of Medicine and Medical Jurisprudence at the University of Richmond. Though this was his first taste of security he left this post at the end of one year, having discovered that they wanted of him only didactic lectures, sufficient to allow the students to pass examinations, and not the results of experimental research. He also found the atmosphere of this southern city, close to the outbreak of the Civil War, distasteful to his republican sentiments.

He returned to France and with Charles Robin founded a private laboratory. Familiar names of students who passed through this laboratory are those of Rosenthal, later professor of neuropathology at Vienna, Westphal, Czermak, and Laboullène. This return to France marks a turning point in his career, since he remained in Paris from 1855 to 1859 and apparently enjoyed a certain contentment. His financial difficulties were eased by the revenues from the private laboratory and from patients sent to him by Rayer. More important, he commenced to receive recognition for his scientific work.

His doctrine of crossed sensibility and the unimportance of the posterior columns as the pathway of touch and pain impulses was actively discussed in the decade between 1850 and 1860, and considerable opposition to his views was expressed. The prevailing doctrine, originating with Charles Bell, was that the posterior

columns are a prolongation of the posterior roots and together are the sole conductors of sensory impulses to the brain, and that the anterolateral columns and the adjacent anterior roots are the sole conductors of motor impulses to the muscles. This was the neat sort of topographical theory which with the weight of authority behind it is given up only with greatest of reluctance. There is no question but what his early work on the spinal cord, though in error on some points, gave the picture of the long conducting paths of the spinal cord which we have today. His scientific reputation and the acceptance of his views on the spinal cord were greatly furthered by the report of a commission of the Société de Biologie which he had asked to examine his experiments. On this committee were Bernard and Vulpian; Broca was chairman. Brown-Séquard was given complete support and the highest praise.

It was during this decade that Brown-Séquard founded his first scientific journal. In 1858 he established and assumed the editorship of the *Journal de la physiologie de l'homme et des animaux*. In the first volume are found 33 articles from his own pen, mostly reports of experiments, one of which is an article on the necessity of the adrenal glands to the continuance of life, a discovery which he had announced in 1856, the year after Addison's clinical monograph. The history of this journal is briefly this. It appeared for six volumes, the last one being published from England. Publication ceased when he went to America in 1863, but on his return to Paris in 1868 a similar journal was founded and edited in collaboration with Vulpian and Charcot. This journal bore the name *Archives de physiologie normale et pathologique* and from 1889 onwards he was, for a number of years, the sole editor. In 1898 the name was changed from *Archives* to *Journal de physiologie et de pathologie générale* and as such is today the principal journal of physiology published in France.

In the years around 1860, Brown-Séquard entered what may be called his English period. In May, 1858, he delivered a series of six lectures before the Royal College of Surgeons; these were published in a more extended form in Philadelphia two years later. The National Hospital for the Paralysed and Epileptic in Queens Square, London, was founded in 1859. Brown-Séquard was chosen as one of the two original honorary physicians, a position which he held until 1863. In 1862 he was joined by Hughlings Jackson

serving in the same capacity. This position carried no honorarium but of course greatly facilitated the acquisition of a successful practice. He chafed now not at poverty but at an uncomfortable richness gained at the expense of time he preferred to devote to experimental physiology. It has been seriously suggested that he left London because he was making too much money and feared to demoralize his son by leaving him a wealthy man. At any rate there are several stories from this period of magnificent fees foregone and cases refused. Many honors came to him during these few years. He was made a Fellow of the Royal Society and of the Royal College of Physicians in 1860. In the following year he delivered both the Gulstonian and the Croonian lectures. The rather grisly subject of the latter was *On the relation between muscular irritability, cadaveric rigidity and putrefaction*. He also gave courses of lectures to medical bodies and universities in Liverpool, Dublin, Edinburgh, and Glasgow.

Possibly because he was impatient with practice, or possibly at the urging of his wife, a native of Boston, he returned to the United States in 1864. On June 11, 1864, the Corporation of Harvard University established in the Medical School a Professorship of the Physiology and Pathology of the Nervous System. The Corporation was apparently a little uncertain about this, since the whole compensation was to be derived from fees, and the duties were rather vaguely specified "to be such as may be determined from time to time." This chair he held for three years during which he formed a close friendship with the dominating scientific figure of that time, Louis Agassiz.

There begins now in his life a period of ten years filled with rapid changes of residence between America and France motivated by causes which are not apparent, and unquestionably also causes of great unhappiness. Of his migrations one can only say that they have a fugue-like air, a patterned, cyclic character reminiscent of the return to Mauritius after the death of his mother. They seem to be connected with marriage or with the loss of a wife. Whatever the cause, these shuttlings back and forth were to continue from 1868 to 1878, at which time he realized the ambition of every French physiologist, the chair of experimental medicine at the Collège de France.

From Harvard he returned to Paris, where through the influence

of Rayer and of Agassiz with Napoleon III a provisional chair was created for him in the Faculty of Medicine. The permanency of this post depended among other things upon his naturalization. With this goal almost attained he remarried in 1872, again to an American woman, and gave up this chair to return to New York to practice medicine. This, his fourth American sojourn, was not a happy one, being clouded by illness, by domestic difficulties, and by the ubiquitous financial ones.

In 1878 the death of Claude Bernard vacated the most highly prized chair in physiology that France has to offer. Brown-Séquard was put up for it and was elected. As a part of standing for this election he prepared the *Notice of his scientific works*, numbering 301 titles and abstracts, which is the only existing list of his works; none exists for his popular writing, of which he did a great deal, nor for the years from 1878 to his death in 1894.

His selection for this post, as it well might, brought a period of contentment, of honor, and of scientific achievement. The position admirably suited his temperament. Rather than didactic lecturing designed to equip the students to face their examiners, Magendie and Bernard had established a tradition of a very personal sort of instruction; the lectures often followed no set and comprehensive course but were determined by the immediate interests of the professor. Much time was devoted to demonstrations, which as often as not were the presentations of the professor's most recent discoveries. This tradition built up about the chair of experimental medicine by Magendie, Bernard, and Brown-Séquard is probably as fine as surrounds any of the chairs of physiology in Europe or America, and might well be followed at the present time. Many honors came his way. He was awarded two substantial monetary prizes by the Académie des Science and in 1886 he was elected to succeed Vulpian in the section of medicine of that academy. A year later he was made president of the Société de Biologie, which he had helped found while still a raw young doctor of medicine and to which he had contributed papers over a period of many years. The intimate atmosphere of the Société suited him better than that of the more august Académie. In it came into play the strong strain of affection in his make up, which showed equally in his attitude to the younger scientists and to his contemporaries. The informal discussions, often heated, gave play to his passionate

absorption in experimental physiology and to his all-embracing imagination.

His life as it approached its end was marked by a tranquillity foreign to it. However, the determinism of human personality is rarely escaped. In 1889 he announced experiments conducted upon himself purporting to prove that testicular extracts will stay the progress of senility. This announcement is a remarkable document and has the flavor which still makes the *Compte rendu* so eminently readable. These rejuvenation experiments immediately caused the greatest excitement in both the scientific and the popular press. Great scepticism and even suspicions of charlatanism were expressed. They were meat for the sensational press, which quickly dubbed his orchitic extract "THE ELIXAR OF LIFE" and exploited it in the familiar pattern. The following title of a journalistic effort is probably typical:

The Elixir of Life. The quack spider and the deluded flies.

Lamb's blood, cutaneously injected, will renew the vigor of youth in the oldest veins, and prolong life indefinitely.

The measure of the distrust engendered in the minds of his fellow scientists is shown by the following discordant note among the gentle tones characteristic of obituaries. Cremer, in an obituary of Brown-Séquard, wrote:

It is unfortunate that he was not able to follow more closely the path of science in the evening of his life. The fantastic researches of his last years (the action of testicular extracts), though perhaps not without any profit to science, are virtually to be designated outright errors of senility.

He died April 1, 1894, a few months after the death of his third wife. Like so many physicians he was a victim of the type of disorder which he had studied. An undiagnosed cerebral attack was the cause of death. The symptoms, which he himself studied and described in letters to his friends within a few days of his death, included a complete loss of vision in the left periphery of the two retinas, vertigo, failure of memory, difficulty in speaking, and paralysis of the arm. It is characteristic of the man that until a few days before his death he refused to go to bed, insisted on writing daily to his friends, and even moved about the house from room to room although he could accomplish this only by going on "all fours."

The appraisal of the work of Brown-Séquard, because of its enormous complexity of subject matter and its fragmentary character, is extremely difficult. We spoke of a parallel between the life of Bernard and Brown-Séquard. This parallel ended with the commencements of their careers because in fact they were of fundamentally different types of mind and personality. Gley, who gives the best commentary on the work of Brown-Séquard, brings out the difference very clearly. He points out that there are two types of scientific intellect. The one type of scientist, endowed with critical intelligence and possessing to a high degree the faculty of minute analysis, is accustomed to follow tenaciously the study of a phenomenon to completion, holding himself strictly to the precise determination of its conditions, and satisfied only when his work is completed. The other type, endowed with an imagination given over to several ideas at a time, hastens to submit all of them to experimental verification at the same time, captivated as he already is with still newer experiments; time he lacks for an extended and rigorous analysis of the facts; it is enough that he has proved their existence; and so he pushes on rapidly towards unknown truths of which he has only an intuition. To Gley, Claude Bernard was one of those rare geniuses who combine both spirits; Brown-Séquard he believes exemplified the second. Gley continues,

Life is short; and yet it is necessary often to find means of living; experiments are difficult and long. Because these fertile spirits have a more lively sense of the pressure of time, they allow themselves to be pushed onwards by their continually changing ideas. The hours which they take from necessary tasks, they give over, instinctively, to new researches rather than to establishing definitely, so laboriously, the truths they deem already acquired. Brown-Séquard was one of the greatest discoverers of facts that the world has ever seen.

There seems no question but what his work suffered from the restlessness and impatience of the man; whether this would have been otherwise if exterior circumstances had favored a more settled and protected existence such as Bernard enjoyed is a matter for speculation. I doubt it. Whether more strictly controlled and ordered efforts would have brought more could be argued; but they could have brought less because after all his four most valuable observations were made in four separate provinces of physiology. Nevertheless, his proof that the adrenal glands are necessary for

life, had it been followed by an analysis of the cause, might have advanced the knowledge of that gland by a quarter or half a century.

To read through the abstracts of his work up to 1878 is a curious experience. One finds so much of value intermixed with much which from our present point of view must be regarded as the grossest of errors and the most errant of nonsense. We find his demonstration of the crossed sensory symptoms combined with ipsilateral paralysis; the discovery of the sensory function of the lateral columns; the necessity of the adrenals to life; stimulation of the cervical sympathetic; the early attempts at endocrine replacement therapy. We find lesser studies which gain our approval: the first use of Weber's esthesiometer in the neurological clinic, experiments directed against the trophic theory of nerves, etc. Intermingled with these are studies of the most bizarre character. We find him espousing many of the "lost causes" of the 19th century; the regeneration of the spinal cord; the inheritance of artificially induced lesions of the nervous system. Running throughout is a consistent vein of studies purporting to show remote pathological manifestations, degenerations, hypertrophies, hemorrhages, from lesions of almost all portions of the nervous system, central, peripheral, and autonomic. A parallel theme is that a lesion of one part of the nervous system will produce phenomena of augmentation and inhibition in all others. This is his doctrine of "*inhibition and dynamogenesis*." This belief led him into a controversy with Charcot in which he attacked the doctrine of localization of function in the nervous system. Since one part of the nervous system can, if diseased, disarrange all other parts there is a type of equipotentiality, not of the cerebral cortex alone, but of the whole nervous system. One cannot help but wonder what exactly he saw and whether in some of his experiments he might not have involved the hypothalamus and produced true pathological phenomena.

A notable feature of his work is the absence of laws which transcend and which unify observations. It is not that he merely observed and refrained from generalizing. He generalized freely enough but his laws are ones of diversity rather than unification. He was more impressed with the diversity of nature than by its orderliness, and one cannot escape the belief that he accepted the diversity of his experimental results as the diversity of nature. He rarely attempted to seek out the common and consistent factors

beneath the variability. Rather he lumped the chaotic observations into laws such as his principle of "inhibition and dynamogenesis" which is merely a statement that almost anything can happen. It is a kind of "nature is as nature does," that which has happened is truth; it is a logical position to take, in an inverted sort of way, once analysis is rejected.

Perhaps the character of his thought processes can best be conveyed by giving his views on vision, which is the sort of thing that led him to deny cortical localization, to be skeptical of neurosurgery and even of localizing brain disorders by neurological examination. He maintained that "a disease in one-half of the brain can produce hemianopia either of both eyes or one, or in the corresponding or the opposite halves of the retina, or a complete amaurosis of either of the two eyes or of both together."

If we view his work generously we must recognize him as a great figure of the 19th century physiology to whom we owe much. If we view him harshly as his contemporaries often did we can believe, since he made many claims and described a hundred phenomena, that he was forced to be correct a few times almost by chance. In any case we must regret that he repudiated several of his most important discoveries.

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